SCIENCE

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*THE ATOMIC THEORY.**

ONE hundred years ago, on October 21, 1803, John Dalton gave this society the first announcement of his famous atomic theory. It was only a slight preliminary notice, a mere note appended to a memoir upon another subject, and it attracted little or no attention. In 1804 Dalton communicated his discovery to Dr. Thomas Thomson, who at once adopted it in his lectures, and in 1807 gave it still wider publicity in a textbook. A year later Dalton published his 'New System of Chemical Philosophy,' and since then the history of chemistry has been the history of the atomic theory. To celebrate Dalton's achievement, to trace its influence upon chemical doctrine and discovery, is the purpose of my lecture. It is an old story, and yet a new one; for every year adds something to it, and the process of development shows no signs of nearing an end. A theory that grows, and is continually fruitful, can not be easily supplanted. Despite attacks and criticisms, Dalton's generalization still holds the field; and from it, as from a parent stem, spring nearly all the other accepted theories of chemistry.

Every thought has its ancestry. Let us briefly trace the genealogy of the atomic theory. In the very beginnings of phi-

* The Wilde lecture before the Manchester Philosophical Society, delivered May 19, 1903.

losophy men sought to discover the nature of the material universe, and to bring unity out of diversity. Is matter one thing or many? Is it continuous or discrete? These questions occupied the human mind before recorded history began, and their vitality can never be exhausted. Final answers may be unattainable, but thought will fly beyond the boundaries of knowledge, to bring back, now and then, truly helpful tidings.

To the early Greek philosophers we must turn for our first authentic statements of an atomic theory. Other thinkers in older civilizations, doubtless, went before them; perhaps in Egypt or Babylonia, but of them we have no certain knowledge. There is a glimpse of something in India, but we can not say that Greece drew her inspiration thence. For us Leucippus was the pioneer, to be followed later by Democritus and Epicurus. Then, in lineal succession, came the Roman, Lucretius, who gave to the doctrine the most complete statement of all. In the thought of these men the universe was made up of empty space, in which swam innumerable atoms. These were inconceivably small, hard particles of matter, indivisible and indestructible, of various shapes and sizes, and continually in motion. From their movements and combinations all sensible matter was derived. Except that the theory was purely qualitative and non-mathematical in form, it was curiously like the molecular hypothesis of modern physics, only with an absolute vacuum where an intermediary ether is now assumed. This notion of a vacuum was repellant to many minds; to conceive of a mass of matter so small that there could be none smaller was unreasonable; and hence there arose the interminable controversy between plenists and atomists which has continued to our own day. It is, however, essentially a metaphysical controversy, and some writers have ascribed it to a peculiar distinction between two classes of minds. The arithmetical thinker deals primarily with number, which is, in its nature, discontinuous, and to him a material discontinuity offers no difficulties. The geometer, on the other hand, has to do with continuous magnitudes, and a limited divisibility of anything in space is not easy for him to conceive. But be this as it may, the controversy was one of words rather than of realities, and its intricacies have little interest for the scientific student of to-day. It is always easier to reason about things as we imagine they ought to be, than about things as they really are, and the latter procedure became practicable only after experimental science was pretty far advanced. The Greeks were deficient in physical knowledge, and, therefore, their speculations remained speculations only, mere intellectual gymnastics of no direct utility to mankind. They sought to determine the nature of things by the exercise of reason alone, whereas science, as we understand it, being less confident, seeks mainly to coordinate evidence and to discover the general statement which shall embrace the largest possible number of observed relations. The man of science may use the metaphysical method as a tool, but he does so with the limitations of definite, verifiable knowledge always in view. Intellectual stimulants may be used temperately, but they need not be discarded altogether.

From the time of Lucretius until the seventeenth century of our era, the atomistic hypothesis received little serious attention. The philosophy of Aristotle governed all the schools of Europe, and scholastic quibblings took the place of real investigation. All scholarship lay under bondage to one master mind, and it was not until Galileo let fall his weights from the

leaning tower of Pisa that the spell of the Stagirite was broken. Experimental science now came to the fore, and it was seen that even Aristotelian logic must verify its premises. The authority of evidence began to replace the authority of the schools.

Early in the seventeenth century the atomic philosophy of Epicurus was revived by Gassendi, who was soon followed by Boyle, by Newton and by many others. One other important step was taken also. Boyle, in his 'Sceptical Chymist,' gave the first scientific definition of element, a conception which was more fully developed by Lavoisier later, but which received its complete modern form only after Davy had decomposed the alkalies and shown the true nature of chlorine. Without this preliminary work of Boyle and Lavoisier, Dalton's theory would hardly have been possible. An elementary atom can be given no real definition unless we have some notion of an element to begin with. But the strongest impulse came from Newton, who accepted atomism in clear and unmistakable terms. Coming before Newton, Descartes had rejected the atomic hypothesis, holding that there could be no vacuum in the universe, and making matter essentially synonymous with extension. True, Descartes, in his famous theory of vortices, imagined whirling particles of various degrees of fineness; but they were not atoms as atoms and molecules are now conceived. It may be dangerous to pick out landmarks in history and to assert that such and such a movement began at such and such a time. Nevertheless, we may fairly say that the turning point in physical philosophy was Newton's discovery of gravitation, for that indicated mass as the fundamental property of matter. For any given portion of matter which we can segregate and identify, extension is variable and mass is constant; when that conclusion was established, the dominance of atomism became inevitable. Boyle, Newton and Lavoisier were legitimate precursors of Dalton, but whether Boscovich should be so considered is more than doubtful. His points of force were too abstract a conception to admit of direct application in the solution of real problems. Dalton certainly owed nothing to Boscovich, and would just as surely have developed his theory had the brilliant Dalmatian never written a line.

To Boyle and Newton the atomic hypothesis was a question of natural philosophy alone; for, in their day, chemistry, as a quantitative science, had hardly begun to exist. Attempts were soon made, however, to give it chemical application, and the first of these which I have been able to find was due to Emanuel Swedenborg. This philosopher, whose reputation as a man of science has been overshadowed by his fame as a seer and theologian, published in 1721 a pamphlet upon chemistry, which is now more easily accessible in an English translation of relatively recent date.* It consists of chapters from a larger unpublished work, and really amounts to nothing more than a sort of atomic geometry. From geometric groupings of small, concrete atoms, the properties of different substances are deduced, but in a way which is more curious than instructive. Between the theory and the facts there is no obvious relation. The book was absolutely without influence upon chemical thought or discovery, and, therefore, it has escaped general notice. It is the prototype of a class of speculative treatises, considerable in number, some of them recent, and all of them futile. They represent efforts which were premature, and for which the

* 'Some specimens of a work on the Principles of Chemistry with other treatises.' London, 1847. Originally published at Amsterdam, in Latin.

fundamental support of experimental knowledge was lacking.

In 1775, Dr. Bryan Higgins, of London, published the prospectus of a course of lectures upon chemistry, in which the atomic hypothesis was strongly emphasized. It was still, however, only an hypothesis, quite as ineffectual as Swedenborg's attempt, and it led to nothing. Dr. Higgins recognized seven elements; earth, water, alkali, acid, air, phlogiston and light; each one consisting of 'atoms homogeneal,' these being 'impenetrable, immutable in figure, inconvertible,' and all 'globular, or nearly so.' He speculated upon the attractions and repulsions between these bodies, but he seems to have solved no problem and to have suggested no research. William Higgins, on the other hand, whose work appeared in 1789, showed more insight into the requirements of true science, and had some notions concerning definite and multiple proportions. His conception of atomic union to form molecules was fairly clear, but the distinct statement of a quantitative law was just beyond his reach. In 1814, however, when Dalton's discoveries were widely known and accepted, Higgins published a reclamation of priority.* In this, with much bitterness, he claims to have completely anticipated Dalton, a claim which no modern reader has been able to allow. In Robert Angus Smith's 'Memoir of John Dalton and History of the Atomic Theory,'† the work of Bryan and William Higgins is quite thoroughly discussed, and, therefore, we need not consider the matter any more fully now. We see that atomic theories were receiving the attention of chemists long before Dalton's time, although none of them went much beyond the speculative stage, or was given serviceable form. They were dim foreshadowings of science; nothing more.

In order that a new thought shall be acceptable, certain prerequisite conditions must be fulfilled. If the ground is not prepared, the seed can not be fruitful; if men are not ready, no harvest will be reaped. Only when the time is ripe, only when long lines of evidence have begun to converge, can a new theory command attention. Dalton's opportunity came at the right moment, and he knew how to use it well. Elements had been defined; the constancy of matter was established; pneumatic chemistry was well developed, and great numbers of quantitative analyses awaited interpretation. The foundations were ready for the master builder, and Dalton was the man. His theory could at once be tested by the accumulated data, and when that had been done it was found to be worthy of acceptance.

It is not my purpose to discuss in detail the processes of Dalton's mind. The story is told in his own note-books, which have been given to the public by Roscoe and Harden,* and it has been sufficiently discussed by others. We now know that Dalton was thoroughly imbued with the corpuscular ideas of Newton, and that, when studying the diffusion of gases, he was led to the belief that the atoms of different substances must be different in size. Upon applying this hypothesis to chemical problems, he discovered that these differences were in one sense measurable, and that to every element a single, definite,

* 'Experiments and Observations on the Atomic Theory and Electrical Phenomena.' By William Higgins, Esq., etc., Dublin, 1814.

† *Memoirs of the Literary and Philosophical Society of Manchester*, Second Series, Volume 13, 1856.

* 'A New View of the Origin of Dalton's Atomic Theory,' etc. By Sir Henry E. Roscoe and Arthur Harden. London, 1896.

See also Debus, in *Zeits. Physikal. Chem.*, Bd. 20, p. 359, and a rejoinder by Roscoe and Harden in Bd. 22, p. 241.

combining number, the relative weight of its atom, could be assigned. From this, the law of definite proportions logically followed, for fractions of atoms were inadmissible; and the law of multiple proportions, which Dalton worked out experimentally, completed the generalization. The conception that all combination must take place in fixed proportions was not new, and, indeed, despite the objections of Berthollet, was generally assumed; but the atomic theory gave a reason for the law and made it intelligible. The idea of multiple proportions had also occurred, although incompletely, to others; but the determination of atomic weights was altogether original and novel. The new atomic theory, which figured chemical union as a juxtaposition of atoms, coordinated all of these relations, and gave to chemistry, for the first time, an absolutely general quantitative basis. The tables of Richter and Fischer, who preceded Dalton, dealt only with special cases of combination, but they established regularities which rendered easier the acceptance of the new and broader teachings. The earlier atomic speculations were all purely qualitative, and incapable of exact application to specific problems; Dalton created a working tool of extraordinary power and usefulness. Between the atom of Lucretius and the Daltonian atom the kinship is very remote.

Dalton was not a learned man, in the sense of mere erudition, but perhaps his limitations did him no harm. Too much learning is sometimes in the way, and clogs the flight of that imagination by which the greatest discoveries are made. The man who could not see the forest because of the trees was a good type of that scholarship which never rises above petty details. It may compile encyclopædias, but it can not generalize. In some ways, doubtless, Dalton was narrow, and he failed to recognize the improvements which other men soon introduced into his system. The chemical symbols which he proposed were soon supplanted by the better formulæ invented by Berzelius, and his views upon the densities of gases were set aside by the more exact work of Gay Lussac, which Dalton never fully appreciated. As an experimenter he was crude, and excelled by several of his contemporaries; his tables of atomic weights, or rather equivalents, were only rough approximations to the true values. These defects, however, are only spots upon the sun, and in no wise diminish his glory. Dalton transformed an art into science, and his influence upon chemistry was never greater than it is to-day. The truth of this statement will appear when we trace, step by step, the development of chemical doctrine. The guiding clue, from first to last, is Dalton's atomic theory.

Although Dalton first announced his theory in 1803, the publication of his 'System' in 1808 marks the culmination of his labors. The memorable controversy between Proust and Berthollet had by this time exhausted its force, and nearly all chemists were satisfied that the law of definite or constant proportions must be true. The idea of multiple proportions was also easily accepted; and as for the combining numbers, they, after various revisions, came generally into use. The atomic conception, however, made its way more slowly, for the fear of metaphysics still governed many acute minds. Davy especially was late in yielding to it, but in time even his conversion was effected. Thomson, as we have already noted, was the earliest and most enthusiastic disciple of the new system, and Wollaston, although cautiously preferring the term 'equivalent' to that of atomic weight, made useful contributions to the theory. These names mark the

childhood of the doctrine, before its vigorous growth had thoroughly begun.

The development of the atomic theory followed two distinct lines, the one chemical, the other physical, in direction. On the chemical side the leader was Berzelius, who began in 1811 the publication of his colossal researches upon definite proportions. At first he seems to have been influenced by Richter rather than by Dalton, but that bias was only temporary. For more than thirty years Berzelius continued these labors, inventing symbols, establishing formulæ and determining atomic weights. He, above all other men, made the atomic theory applicable to general use, a universal tool suited to practical purposes. Turner, Penny, Erdmann and others did noble work of the same order, but Berzelius overshadowed them all. Throughout his long career he was almost the dictator of chemistry.

It was on the physical side, however, that the theory of Dalton was most profoundly modified. First came the researches of Gay Lussac, who in 1808 showed that combination between gases always took place in simple relations by volume, and also that all gaseous densities were proportional either to the combining weights of the several substances, or to rational multiples thereof. In 1811 Avogadro generalized the new evidence, and brought forward the great law which is now known by his name. Equal volumes of gases, under like conditions of temperature and pressure, contain equal numbers of molecules. Mass and volume were thus covered by one simple expression, and both were connected with the weights of the fundamental atoms. Avogadro, moreover, distinguished clearly between atoms and molecules, a distinction which is of profound importance to chemistry, although it is not always properly appreciated by students of physics. The molecule of to-day, which is usually, but not always, a cluster of atoms, is identical with the atom of the pre-Daltonian philosophers; while the chemical unit represents a new order of divisibility which the ancients could never have imagined. A molecule of water was easily conceived by them, but its decomposition into smaller and simpler particles of oxygen and hydrogen, the chemical atoms, was far beyond the range of their knowledge. That the distinction is not always borne in mind by physicists is illustrated by the fact that in Clerk Maxwell's article 'Atom,' in the 'Encyclopædia Britannica,' Dalton is not even mentioned, and that the phenomena there selected for discussion are molecular only. Maxwell was surely not ignorant of the difference between atoms and molecules, but his knowledge had not reached the point of complete realization. His thought was of molecules, and so Maxwell unconsciously neglected the real subject of his chapter, the atom. Of late years many essays upon the atomic theory have been written from the physical side, and few of them have been free from this particular ambiguity. At first, a similar error was committed by chemists, who paid small attention to Avogadro's law, and so the latter failed to exert much influence upon chemical thought until more than forty years after its promulgation. The relation discovered by Dulong and Petit in 1819, that the specific heat of a metal was inversely proportional to its atomic weight, was more speedily accepted; but even this law did not receive its full application until many years later. To apply either of these laws to chemical theory involved a clearer discrimination between atomic weights and equivalents than was possible at the beginning. A long period of doubt and controversy was to work itself out before the full force of the physical evidence could be

appreciated. Mitscherlich's researches upon isomorphism were more fortunate, and gave immediate help in the determination of atomic weights and the settlement of formulæ. For the moment we need only note that the chemical atom was the underlying conception by means of which all these lines of testimony were to be unified.

From Dalton and Gay Lussac to Frankland and Cannizzaro was a time of fermentation, discussion and discovery. In chemistry, contrary to the saying of the preacher, there were many new things under the sun, and some of the discoveries were most suggestive. First it was found that certain groups of atoms could be transferred from compound to compound, almost as if they were veritable elements; and radicles such as ammonium, cyanogen and benzoyl were generally recognized. I say 'groups of atoms' advisedly, for as such they were regarded, and they could hardly have been interpreted otherwise. Then came the discovery of isomerism; of the fact that two substances could be strikingly different, and yet composed of the same elements in exactly the same proportions. This was only explicable upon the supposition that the atoms were differently arranged within the isomeric molecules, and it led investigators more and more to the study of chemical or molecular structure. Without the atomic theory the phenomena would have been hopelessly bewildering; with its aid they were easy to understand, and fertile in suggestions for research. Still another link in the chain of chemical reasoning was forged by Dumas, when he proved that the hydrogen of organic compounds was often replaceable, atom for atom, by chlorine. Sometimes the replacement was complete, sometimes it was only partial, and the latter cases were the most significant. In acetic acid, for example, one, two or three fourths of the hydrogen could be successively replaced, but the last fourth was permanently retained. Hydrogen, then, was combined in acetic acid in two different ways, one part yielding its place to chlorine, the other being unaffected. This behavior was soon found to be by no means exceptional; indeed, it was very common, and it opened a new line of attack upon the problems of chemical constitution. The existence of radicles, the formation of isomers, and the substitution of one element by another, were facts which strengthened the atomic theory and seemed to be incapable of reasonable interpretation upon other terms. Their connection with one another, however, was not well understood, and wearisome discussions preceded their coordination under one general law.

With the tedious controversies which distracted chemists between 1830 and 1850, we have nothing now to do; they were important in their day, but they do not come within the scope of the present argument. Theory after theory was advanced, prospered for a time, and then decayed; and chemical literature is crowded with their fossil remains. Each one, doubtless, indicated an advance in knowledge, but each one also exaggerated the importance of some special set of relations, and so overshot the mark. During this period, however, Faraday discovered the law of electrolysis which is now known by his name, and the chemical equivalents were thereby given another extension of meaning. The electrochemical theories of Berzelius had fallen to the ground, but Faraday's law came as a permanent addition to the physical side of chemistry.

During the sixth decade of the nineteenth century, two important forward steps were taken. The kinetic theory of gases gave new force to Avogadro's law, and made its complete recognition by chem-

ists necessary. Atoms, molecules, equivalents and atomic weights needed to be more sharply defined, and in this work many chemists shared. Berzelius had proposed a system of atomic weights which differed, except in the value taken for its base, but little from the one now in use. This was abandoned for a table devised by Gmelin, in which the laws of Avogadro and of Dulong and Petit were almost if not entirely ignored. Laurent and Gerhardt attempted to reform the system, but it was left for Cannizzaro, in 1858, to succeed. By doubling some of the currently accepted atomic weights, order was introduced into the prevailing chaos, and the chemical constants were brought into harmony with the physical laws. The modern atomic weights and our present chemical notation may be dated from this time, even though the preliminary anticipations of them were neither few nor inconspicuous.

The second great step forward was accomplished through the labors of several men. Frankland and Kekulé were foremost among them, but Couper, Odling, Williamson, Wurtz and Hofmann all contributed their share to the upbuilding of a new chemistry, of which the doctrine of valency was the cornerstone. A new property of the chemical atom was brought to light, and structural or rational formulæ became possible. Each atom was shown to have a fixed capacity for union with other atoms, a capacity which could be given numerical expression; and from this discovery important consequences followed. An atom of hydrogen unites with one other atom only; the atom of oxygen may combine with two; that of nitrogen with three or five; while carbon has capacity for four. All unions of atoms to atoms within a molecule are governed by conditions of this order, and the limitations thus imposed determine the possibilities of combination in a given class of compounds. In organic chemistry the conception of valency has been most fruitful, and it has shown the prophetic power which is characteristic of all good theories. It explains radicles and isomers; it predicts whole classes of compounds in advance of their actual discovery; and it has guided economic investigations from which great industries have sprung. The former partial theories regarding chemical constitution fell into their proper places under the new generalization, for that was broad enough to comprehend them all. All constitutional chemistry depends upon this property of the atoms, and any other adequate foundation for it would be difficult to find.

I have said that the discovery of valency explained the phenomena of isomerism. Indeed, it enabled chemists to foresee the existence of new isomers, and it established the conditions under which such compounds could exist. And yet, in one direction at least, its power was limited, and substances were found which the theory could not interpret. Tartaric acid, for example, exists in two modifications, differing in crystalline form and in their action upon polarized light. One acid was dextrorotatory, the other lævorotatory, while a mixture of the two in equal proportions was neutral to the polarized beam, and gave no rotation at all. Their crystals exhibited a similar difference in the arrangement of certain planes, one set being right-handed, the other left-handed; and each crystal resembled its isomer like a reflection in a mirror, alike, but reversed. For a long time this physical isomerism, as it was called, remained inexplicable, for the rules of valency gave to both molecules the same structure, and offered no hint as to the cause of the difference. Structural formulæ, however, said nothing of the arrangement of the atoms in tridimensional space, and it was

soon suspected that the root of the difficulty was here. The mere linking of the atoms with one another could be represented in a single plane, but that was obviously an imperfect symbolism.

In 1874 van't Hoff and Le Bel, working independently of each other, suggested a solution of the problem. One simple assumption was enough; merely that the quadrivalent carbon atom was essentially a tetrahedron, or, more precisely, that its four units of chemical attraction were exerted, from a common center, in the direction of four tetrahedral angles. Atoms of that kind could be built up into structures in which right-handedness and left-handedness of arrangement appeared, provided only that each one was united with four other atoms or groups all different in nature. Stereo-chemistry was born, the anomalies vanished, and many new substances showing optical and crystalline properties analogous to those of tartaric acid were soon prepared. The theory of van't Hoff and Le Bel was fertile, and therefore it was justified; it interpreted another set of phenomena, but, in order to do so, something like atomic form had first to be assumed. It was only a new extension of Dalton's atomic theory, but it has suggested a future development of extraordinary significance. If we can determine, not merely the linking of the atoms, but also their arrangement in space, we should be able, sooner or later, to establish a connection between chemical composition and crystalline form. The architecture of the molecule and the architecture of the crystal must surely, in some way, be related. But the problem is exceedingly complex, and we may have to wait many years before we reach its solution. The atomic theory still has room to grow.

Let us now turn back in time, and consider another phase of our subject. In 1815 Prout suggested that the atomic weights of all the elements were even multiples of that of hydrogen. It was only a speculation on the part of Prout, and yet it led to important consequences, for it opened a discussion upon the nature of the chemical elements, and it pointed to hydrogen as the primal matter of the universe. Prout's hypothesis, therefore, became a subject of controversy; it found many supporters and also many antagonists; but, fortunately, one aspect of it was capable of experimental investigation. Some of the most exact and elaborate determinations of atomic weight have been made with the direct purpose of testing the truth or falsity of Prout's speculation, and science thereby has been notably enriched. The marvelous researches of Stas, for instance, had this specific object in view. The verdict was finally unfavorable to Prout; at least, the best measurements fail to support his idea; but it still has advocates who believe that the experimental data are vitiated by unknown errors and that future investigations will reverse the decision. In science there is no court of last appeal.

Prout's hypothesis, then, stimulated the determination of atomic weights, and so helped us to a more accurate knowledge of them. It also led to a search for other relations between these constants, and thus paved the way for important discoveries. Döbereiner, Kremers, Dumas, Pettenkofer, Cooke and many other chemists published memoirs upon this theme, but not one of them was general or conclusive.* Groups of elements were compared and relations were brought to light, but an exhaustive study of the question was hardly possible until after Cannizzaro had revised the

* A very full account of these attempts is given in Venable's book, 'The Development of the Periodic Law.' Published at Easton, Pennsylvania, in 1896.

atomic weights and indicated their proper values.

In 1865, Newlands presented before the London Chemical Society a communication upon the law of octaves, in which he showed that the elements, when arranged in the order of their atomic weights, exhibited a certain regular recurrence of properties. Unfortunately, his views were not given serious attention, and even met with ridicule, but they contained the germ of the great truth. It was reserved for the Russian, Mendeléeff, four years later, to completely formulate the famous periodic law.

Mendeléeff arranged the elements in tabular form, still following the order of their atomic weights. A periodic variation of their properties, including the property of valency, at once became evident; and although the scheme was, and still is, open to some criticism, its importance could hardly be denied. In the table, certain gaps appeared, presumably belonging to unknown elements, and for three of these some remarkable predictions were made. The hypothetical elements were described by Mendeléeff, their atomic weights were assigned and their physical properties foretold, and in due time the prophecies were verified. The three metals gallium, scandium and germanium have since been discovered, and they correspond very closely with Mendeléeff's anticipations. His general conclusion was that all of the physical properties of the chemical elements are periodic functions of their atomic weights, and this conclusion, I think, is no longer seriously doubted. The curves of atomic volumes and melting points which Lothar Meyer afterwards constructed give strong support to this view.

The periodic system, then, gives to the numbers discovered by Dalton a much more profound significance than he ever imagined, and is destined to connect a great mass of physical data in one general law. That law we now see, 'as in a glass, darkly'; its complete mathematical expression is yet to be found, but I believe that it will be fully developed within the near future. We may have a spiral curve to deal with, as in the schemes proposed by Stoney or by Crookes, or else a vibratory expression like that suggested by Emerson Reynolds in his presidential address before the Chemical Society last year; but in some form the periodicity of the elements must be recognized, and one set of relations will connect them all. In the arrangement proposed by Reynolds the inert gases, the elements of zero valency, appear at the nodes of a vibrating curve, a circumstance which gives this method of presentation a peculiar force. But for the consideration of physical properties the curves drawn by Lothar Meyer seem likely to be the most useful. In one respect, however, the periodic system is still defective; it fails to take adequately into account the numerical relations between the atomic weights, a phase of the problem which should not be ignored. Such relations exist; some of them have been indicated by your distinguished fellow member, Dr. Wilde; and, elusive as they may seem to be, they are surely not meaningless. The final law must cover the entire ground, and then atomic weights, physical properties and valency will be completely correlated. Prout's hypothesis is discredited, and yet it may prove to be a crude first approximation to some deeper truth, as the probability calculations of Mallet* and of Strutt† would seem to indicate. The approaches of the atomic weights to whole numbers are too close and too frequent to be regarded as purely accidental. But this is aside from

* *Phil. Trans.*, Vol. 171, 1881, p. 1003.

† *Phil. Mag.* (6), 1, p. 311.

our main question. The real point to note is that the physical properties of the elements are all interdependent, and that the fundamental constants are the atomic masses.

Do I seem to exaggerate? Then look for a moment at the present condition of physical chemistry, and see how moderate my statements really are. We have not only the laws already mentioned, of Avogadro, of Dulong and Petit, of Faraday and of Mendeléeff, but also a multitude of relations connecting the physical constants of bodies with their chemical character. Even the wave-lengths of the spectral lines are related to the atomic weights of the several elements, as has been shown by the researches of Runge and his colleagues, of Rummel,* and of Marshall Watts.† If we try to study the specific gravity of solids or liquids, the only clues to regularity are furnished by the atomic ratios. Atomic and molecular volumes give us the only approximations to anything like order. Similarly, we speak of atomic and molecular refraction, of molecular rotation for polarized light, of molecular conductivity and the like. In Trouton's law, the latent heat of vaporization of any liquid becomes a function of the molecular weight. And, finally, all thermochemical measurements are meaningless until they have been stated in terms of gram molecular weights; then system begins to appear. Chaos rules until the atomic or molecular weight is taken into account; with that considered, the reign of order begins.

Even to the study of solutions the same conditions apply. Substances in solution exert pressure, and in this respect they closely resemble gases. Van't Hoff has shown that equal volumes of solutions, having under like conditions equal osmotic pressures, contain equal numbers of molecules, and thus Avogadro's gas law is curiously paralleled. The two laws are even equivalent in their anomalies. The abnormal density of a gas is explained by its dissociation, and the variations from van't Hoff's law are explicable in the same way. The theory of ionic or electrolytic dissociation, proposed by Arrhenius, shows that certain substances, when dissolved, are split up into their ions, and through this conception the analogy between gases and solutions is made absolutely complete. The ions, however, are atoms or groups of atoms; and just as Avogadro's law is applied to the determination of molecular weights among gases, so van't Hoff's rules enable us to measure the molecular weights of substances in solution. The atom, the molecule, and the molecular weight enter into all of these new generalizations. In short, if we take the atomic theory out of chemistry, we shall have little left but a dust-heap of unrelated facts.

I have now indicated, briefly and in outline only, the influence of the atomic theory upon the development of chemical thought. Details have been purposely omitted; the salient facts are enough for my purpose, and they make, at least for chemists, an exceedingly strong case. The convergence of the testimony is remarkable, and when we add to the chemical evidence that which is offered by physics, the theory becomes overwhelmingly strong. This side of the question I can not attempt to discuss, but I may in passing just refer to Professor Rücker's presidential address before the British Association in 1901, which covers the ground admirably. The atomic theory has had no better vindication.

And yet, from time to time, we are told that the theory has outlived its usefulness, and that it is now a hindrance rather than a help to science. Some of the objectors

* *Proc. Roy. Soc. Victoria*, Vol. 10, part I., p. 75.

† *Phil. Mag.* (6), 5, 203.

are quite dogmatic in their utterances; some only seek to evade the theory, without going to the extreme of an absolute denial; and still others, more timid, assume an apologetic tone, as if the atom were something like a poor relation, to be recognized and tolerated, but not to be encouraged too far. Now caution is a good thing, if it is not allowed to degenerate into indecision; when that happens, mental obscurity is the result. In science we must have intellectual resting-places; something to serve as a foundation for our thinking; something concrete and tangible in form. No theory is immune against hypercriticism; none is absolute and final; with these considerations borne in mind we may ask whether a doctrine is serviceable or not, and we can use it without fear. When we say that matter, as we know it, behaves as if it were made up of very small, discrete particles, we do not lose ourselves in metaphysics, and we have a definite conception which can be applied to the correlation of evidence and the solution of problems. Objections count for nothing against it until something better is offered in its stead, a condition which the critics of the atomic theory have so far failed to fulfil. They give us no real substitute for it, no other working tool, and so their objections, which are too often metaphysical in character, command little serious attention. Criticism is useful, just so far as it helps to clarify our thinking; when it becomes a mere agent of destruction it loses force.

Broadly speaking, then, the modern critics of the atomic theory have shaken it but little. Still, some serious attempts have been made towards forming an alternative system of chemistry, or at least a system in which the atom shall not avowedly appear. The most serious, and perhaps the most elaborate of these devices was that brought forward in 1866 by Sir Benjamin Brodie,* in his 'Calculus of Chemical Operations,' which he defended later (1880) in a little book entitled 'Ideal Chemistry.' In this curious investigation, Brodie tries to avoid hypotheses and to represent chemical acts as operations upon the unit of space by which weights are generated. This notion is a little difficult to grasp, but Brodie's procedure was perfectly legitimate. His one fundamental assumption is that hydrogen is so generated by a single operation, and upon this he erects a system of symbols which, treated mathematically, lead to some remarkable conclusions. For instance, chlorine, bromine, iodine, nitrogen and phosphorus become compounds of hydrogen with as many unknown or 'ideal' elements, which no actual analysis has yet identified. That is, the known phenomena of chemistry seem to be less simply interpreted by Brodie's calculus than in our commonly accepted theories, and certain classes of phenomena are not considered at all. It is true that Brodie never completed his work, but it is not easy to see how his notation and reasoning could have accounted for isomerism, much less for the facts which stereochemistry seeks to explain.

Just here we find the prime difficulty of all attempts to evade the atomic theory. Up to a certain point we can easily dispense with it, for we can start with the fact that every element has a definite combining number, and then, without any assumptions as to the ultimate meaning of these constants, we can show that other constants are intimately connected with them. So far, we can ignore the origin of the so-called atomic weight; but the moment we encounter the facts of isomerism or chemical structure, and of the partial substitution of one element by another, our troubles

* *Phil. Trans.*, 1866. A second part in 1877.

begin. The atomic theory connects all of these data together, and gives the mind a simple reason for the relations which are observed. We can not be satisfied with mere equations; our thought will seek for that which lies behind them; and so the anti-theorist fails to accomplish his purpose because he leaves the human mind out of account. The reasoning instrument has its own laws and requirements, and they, as well as the empirical observations of science, must be satisfied. Even in astronomy the law of gravitation is not enough; men are continually striving to ascertain its cause; and no number of failures can prevent them from trying again and yet again to penetrate into the heart of the mystery. In the atomic theory the same tendency is at work, and the very nature of the atom itself, that thing which we can neither see nor handle, has become a legitimate subject for our questionings. Shall we, having gone so far, assume that we can go no farther?

'All roads lead to Rome.' If we accept the atomic theory, we sooner or later find ourselves speculating about the reality of the atom, and at last we come face to face with the old, old problem of the unity or diversity of matter. We can, if we choose, employ the theory as a working tool only, and shut our ears to these profounder questions; but it is not easy to do so. What is the chemical atom? Is all matter ultimately one substance? We may be unable to solve either problem, and yet we can examine the evidence and see which way it points.

I think that all philosophical chemists are now of the belief that the elements are not absolutely distinct and separate entities. In favor of their elementary nature we have only negative evidence, the mere fact that with our present resources we are unable to decompose them into simpler forms. On that side of the argument there is nothing more. On the other hand, we see that the elements are bound together by the most intimate relations, so much so that unknown elements can be accurately described in advance of their discovery, and facts like these call for an explanation. Something belonging to the elements in common seems to underlie them all. If, however, we study the atomic weights, we are forced to observe that the elements do not shade into one another continuously, but that they vary by leaps which are sometimes relatively large, and sometimes quite small. To Mendeléeff this irregular discontinuity is an argument against the unity of matter, or, rather, an indication that the periodic law lends no support to the belief; but such a conclusion is unnecessary. If the fundamental matter, the 'protyle,' as Crookes has called it, is itself discontinuous and atomic in structure, the same property must be shown in all of its aggregations, and so the difficulties seen by Mendeléeff disappear. The chemical atoms become clusters of smaller particles, whose relative magnitudes are as yet unknown.

That bodies smaller than atoms really exist is the conclusion reached by J. J. Thomson* from his researches upon the ionization of gases. According to him, this phenomenon 'consists in the detachment from the atom of a negative ion,' this being 'the same for all gases.' He regards 'the atom as containing a large number of smaller bodies,' which he calls 'corpuscles,' and these are equal to one another. "In the normal atom this assemblage of corpuscles forms a system which is electrically neutral." It must be borne in mind that these conclusions are drawn by Thomson from the study of one class of phenomena,

* *Phil. Mag.* (5), 48, p. 547. Also *Popular Science Monthly*, August, 1901.

and it is of course possible that they may not be finally sustained. Their value to us at the present moment lies in their suggestiveness, and in the curious way in which they reinforce other arguments of similar purport. The possibility that the chemical atoms can be actually broken down into smaller particles of one and the same kind, is, to say the least, startling, but it can not be disregarded. The evidence obtained by Thomson is, so far as it goes, positive, and it is entitled to receive due weight in all discussions of our present problem. It is the first *direct* testimony that we have been able to obtain, all previous evidence being either negative or circumstantial. It may be misinterpreted, but it is not to be pushed aside.

In direct line with the inferences of Thomson are the results obtained by Rutherford and Soddy in their researches upon radio-activity. Here, again, we have a subject so new that all opinions concerning it must be held open to revision, but, so far as we have yet gone, the evidence seems to point in one way. Rutherford and Soddy* have studied especially the emanations given off by thorium, and conclude that from this element a new body is continually generated, in which the radioactivity steadily decays. This loss of emanative power is in some sort of equilibrium with the rate of its formation. When thorium is 'de-emanated,' it slowly regains its emanative power. The emanation is a 'chemically inert gas, analogous in nature to the members of the argon family.' The final conclusion is that radio-activity may be 'considered as a manifestation of subatomic chemical change.' This word 'subatomic' is one of ominous import. It implies atomic complexity, and it also suggests something more. The property of radio-activity is most strikingly exhibited by the metals radium, thorium and uranium; and these have the highest atomic weights of any elements known. If the elements are complex, these are the most complex, and therefore, presumably, the most unstable. Are they in the act of breaking down? Is there a degradation of matter comparable with the dissipation of energy? We can ask these questions, but we may have to wait long for a reply. There is, however, another side to the shield, and the universe gives us glimpses of a generative process, an elementary evolution.

* *Phil. Mag.* (6), 4, pp. 395 and 581.

The truth or falsity of the nebular hypothesis is still an open question. It is a plausible hypothesis, however, and commands many strong arguments in its favor. We can see the nebulæ, and prove them to be clouds of incandescent gas; we can trace a progressive development of suns and systems, and at the end of the series we have the habitable planet upon which we dwell. The nebular hypothesis accounts for the observed condition of things, and is therefore, by most men, regarded as satisfactory. But this is not all of the story. Chemically speaking, the nebulæ are exceedingly simple in composition; the whiter and hotter stars are a little more complex; then come stars like our sun and finally the finished planets with their many chemical elements and their myriads of compounds. Here again we have evidence bearing upon our problem, evidence which led me,* more than thirty years ago, to suggest that the evolution of planets from nebulæ had been accompanied by an evolution of the elements themselves. This thought, stated in a reversed form, has since been developed and amplified by Lockyer, and it is doubtless familiar to you all. In the development of the heavenly bodies we seem to see the

* 'Evolution and the Spectroscope,' *Popular Science Monthly*, January, 1873.

growth of the elements; do we, in the phenomena of radio-activity, witness their decay? This is a startling, possibly a rash speculation, but it rests upon evidence which must be considered and weighed.

We have, then, various lines of convergent testimony, and there are more which I might have cited, all pointing to the conclusion that the chemical atoms are complex, and that elemental matter, in the last analysis, is not of many kinds. That there is but one fundamental substance, is not proved; and yet the probability in favor of such an assumption must be conceded. Assuming it to be true, what then is the nature of the Daltonian atom?

To the chemist, the simplest answer to this question is that furnished by the researches of J. J. Thomson, to which reference has already been made. A cluster of smaller particles or corpuscles satisfies the conditions that chemistry imposes on the problem, their ultimate nature being left out of account. For chemical purposes we need not inquire whether the corpuscles are divisible or indivisible, although for other lines of investigation this question may be pertinent. But no matter how far we may push our analysis, we must always see that something still lies beyond us, and realize that nature has no assignable boundaries. That which philosophers call 'the absolute' or 'the unconditioned' is forever out of our reach.

Through many theories men have sought to get back a little farther. Among these, Lord Kelvin's theory of vortex atoms is perhaps the most conspicuous, and certainly the best known. It presupposes an ideal perfect fluid, continuous, homogeneous and incompressible; portions of this in rotation form the vortex rings, which, when once set in motion by some creative power, move on indestructibly forever. These rings may be single, or linked or knotted together, and they are the material atoms. The assumed permanence of the atom is thus accounted for and given at least a mathematical validity, but we have already seen that the chemical units may not be quite so simple. The ultimate corpuscles, to use J. J. Thomson's words, may be vortex rings; the chemical atom is much more complex. On this theory, chemical union has been explained by supposing that vortices are assembled in rotation about one another, forming groups which are permanent under certain conditions and yet are capable of being broken down. The vortex ring is eternal, its groupings are transitory. This is a plausible and fascinating theory; if only we can imagine the ideal perfect fluid and apply to it the laws of motion; that done, all else follows. Unfortunately, however, the fundamental conception is difficult to grasp and still more difficult to apply. So far, it has done little or nothing for chemistry; it has brought forth no discoveries, nor stimulated chemical research; we can only say that it does not seem to be incompatible with what we think we know. In a certain way it unifies the two opposing conceptions of atomism and plenism, and this may be, after all, its chief merit.

But there are later theories than that of Kelvin, and some of them are most daring. For instance, Professor Larmor regards electricity as atomic in its nature, and supposes that there are two kinds of atoms, positive and negative electrons. These electrons are regarded as centers of strain in the ether, and matter is thought to consist of clusters of electrons in orbital motion round one another. Still more recently, Professor Osborne Reynolds, in his Rede lecture,* has offered us an even more

* 'On an Inversion of Ideas as to the Structure of the Universe.' Cambridge, 1903. The Rede Lecture, delivered June 10, 1902.

startling solution of our problem. He replaces the conventional ether by a granular medium, generally homogeneous, closely packed, and having a density ten thousand times that of water. Here and there the medium is strained, producing what Reynolds calls 'singular surfaces of misfit' between the normally piled grains and their partially displaced neighbors. These surfaces are wave-like in character, and constitute what we recognize as ordinary matter. Where they exist there is a local deficiency of mass, so that matter is less dense than its surroundings; and this, as Reynolds has said, is a complete inversion of the ideas which we now hold. Matter is measured by the absence of the mass which is needed to complete a normal piling of the grains in the medium. In other words, it might be defined as the defect of the universe. The 'singular surfaces' already mentioned are molecules, which may cohere, but can not pass through one another, and they preserve their individuality. Possibly I may misapprehend this theory, for it has been published in a most concise form, and the reasoning upon which it rests is not given in detail. I can not criticize it, but I may offer some suggestions. If matter consists of waves in a universal medium, how does chemical union take place? Shall we conceive of hydrogen as represented by one set of waves and nitrogen as represented by another, the two differing only in amplitude? If so, when they combine to form ammonia there should be either a superposition of one set upon the other, or else a complex system might be found showing interference phenomena. But would not the latter supposition imply a destruction of matter as matter is defined by theory? Could one such wave coalesce with or neutralize another? To conceive of a union of waves without interference is not easy, but the facts of chemical combination must be taken into account. When we remember that compounds exist containing hundreds of atoms within the molecule, we begin to realize the difficulties which a complete theory of matter must overcome. Chemical and physical evidence must be taken together; neither can solve the problem alone. At present, the simplest conception for the mind to grasp is that of an aggregation of particles. Beyond this all is confusion, and mathematical devices can help us only a little. In speaking thus I assign no limit to the revelations of the future; some theory, now before the world, may prove its right to existence and survive; but none such, as yet, can be taken as definitely established. The theory which stands the test of time will not be a figment of the imagination; it must be an expression of observed realities. But enough of speculation; let me, before I close, say a few words of a more practical character.

Dalton's statue stands in Manchester, a fitting tribute to his fame. But it is something which is finished, something on which no more can be done, something to be seen only by the few. As a local memorial it serves a worthy purpose, but Dalton's true monument is in the set of constants which he discovered, and which are in daily use by all chemists throughout the world. Here is something that is not finished; and here Dalton's memory can be still further honored, by good work, good research, honest efforts to increase our knowledge. We have seen that the atomic weights are the fundamental constants of all exact chemistry, and that they are almost as important also to physics; but the mathematical law which must connect them is still unknown. Every discovery along the line of Dalton's theory is another stone added to his monument, and many such discoveries are yet to be made.

What, now, is needed? First, every

atomic weight should be determined with the utmost accuracy, and what Stas did for a few elements ought to be done for all. This work has more than theoretical significance; its practical bearings are many, but it cannot be done to the best advantage along established lines. So far the investigators have been a mob of individuals; they need to be organized into an army. Collective work, cooperative research, is now demanded, and the men who have hitherto toiled separately should learn to pull together. Ten men, working on a common plan, in touch with one another, can accomplish more in a given time than a hundred solitaries. The principles at issue are well understood; the methods of research are well established; but the organizing power has not yet appeared. Shall this be a great institution for research, able to take up the problems which are too large for individuals to handle, or a voluntary cooperation between men who are unselfishly inclined to attempt the work? This question I can not answer; doubtless it will solve itself in time; but I am sure that a method of collective investigation will be found sooner or later, and that then the advance of exact knowledge will be more rapid than ever before. When the atomic weights are all accurately known, the problem of the nature of the elements will be near its solution. Some of the wealth which chemistry has created might well be expended for this purpose. Who will establish a Dalton laboratory for research, and so give the work which he started a permanent home?

F. W. Clarke.

SCIENTIFIC BOOKS.

British Museum (Natural History); First Report on Economic Zoology. By Fred. V. Theobald, M.A.

This is a volume of xxxiv–192 pages, with 18 illustrations, consisting primarily of a series of reports to the Board of Agriculture, of reports and letters to a variety of unofficial correspondents, and of reports to the Foreign Office and the Colonial Office, drawn up by Mr. Theobald during the years 1901–1902. Mr. Theobald has recently been employed by the trustees of the British Museum to assist the director in such work, especially with a view of furnishing the Board of Agriculture with scientific information on Economic Zoology, in accordance with a request made by that department of His Majesty's government.

As may be supposed, the subjects treated have come from all parts of the British Empire and are of more than local interest. The insects mentioned, having especial interest for the American entomologist, are the pear midge, *Diplosis pyrivora* Riley; the mussel scale, *Mytilaspis pomorum;* the apple aphis, *Aphis mali;* the tarnished plant bug, *Lygus pratensis,* attacking chrysanthemums; *Dermestes lardaris;* the bud moth, *Hedya ocellana;* the pear-leaf blister mite, *Eriophyes pyri;* and the Colorado potato beetle which made its appearance in England in 1901 and again in 1902. This last pest appeared in Tilbury dockyard on potato plants on the workmen's allotments. The land was cleared of all potato hulm and the hulm burned with paraffin, at night, on the ground and under the supervision of an officer of the Board of Agriculture; the ground soaked with paraffin, and plowed ten inches deep, after which it was dressed with gas lime, 60 tons per acre. Despite this treatment a few beetles appeared in 1902, but these were promptly collected and destroyed.

While not comparing with the classical reports of the late Miss Ormerod, from an entomological point of view, this is England's first attempt at providing for an official entomologist, and it is to be hoped that it may prove a beginning that will expand until the mother country will no longer continue to be outdone by even her smallest colonies, like Tasmania, Cape Colony and Natal, for illustration. Mr. Theobald might well be wholly employed in this work, and his first report is a good indication that he would prove a most